

Ronald Pethig

Biographical Statement

After earning his B.Sc. in Electronic Engineering at the University of Southampton, Ronald Pethig continued his studies there and completed his Ph.D. in Electrical Engineering. In 1968 he was appointed an ICI (Imperial Chemical Industries) Fellow at the Department of Chemistry, University of Nottingham, where he subsequently obtained a second Ph.D. He then received an appointment to a lectureship at the School of Electronic Engineering Science at the University of Wales, Bangor. In 1986 he was awarded a Personal Chair in the School of Informatics at the University of Wales, Bangor, and two years later was awarded a D.Sc. in Biomolecular Electronics by the University of Southampton. From 1986-1998 he served as Director of the Institute of Molecular and Biomolecular Electronics at Bangor and, from 1991-1993, as Dean of the Faculty of Science and Engineering.

Professor Pethig is a named inventor on thirteen patents and has been active in directing private business ventures in the United States involving biotechnology. He has served on the editorial boards of professional journals, including the editorship of the *Microtechnologies and Microsystems Series*, published by Research Studies Press. He has authored one book (*Dielectric and Electronic Properties of Biological Materials*) and over 200 peer-reviewed articles, and has served on several research councils in the United Kingdom. His honors include the joint Innovation Prize of the Institutes of Electrical and Mechanical Engineering and the British Design Council (1988), and in 1994 the Innovation Awards of the Institute of Physical Sciences in Medicine and the Biological Engineering Society. In Oslo, Norway, in 2001, he became the first recipient of the Herman P. Schwan Award for contributions to the field of Biodielelectrics.

Interview Synopsis

After Professor Pethig wrote to Albert Szent-Györgyi concerning shared research interests, Szent-Györgyi, then in his early eighties, invited him to visit his Woods Hole laboratory. From then on, Pethig was a regular visitor and participant in research projects conducted in Szent-Györgyi's lab. In 1982 he was elected a Corporation Member of the Marine Biological Laboratory at Woods Hole. In this insightful and revealing interview, Professor Pethig describes his first meeting with Szent-Györgyi, Szent-Györgyi's approach to science and his views on the importance of submolecular or "quantum" biology, and Pethig's role, along with Peter Gascoyne, in researching some of Szent-Györgyi's theories. Along the way, Pethig describes some of the factors contributing to Szent-Györgyi's controversial status in the scientific community. As a Nobel Prize winner, Szent-Györgyi was highly respected; but at the same time, some of his ideas and the style with which he expressed them, particularly surrounding his theories of cancer, seemed to offend conservative or even "mainstream" scientists. Pethig offers some useful observations about scientific careers and the "politics" of science as they pertain to Szent-Györgyi's life. Lastly, Pethig discusses Szent-Györgyi's final weeks and hours, his thoughts on cosmology and religion, and his sense of humor.

**National Library of Medicine
Interview with Ronald Pethig
Conducted on August 20, 2004, by Adrian Kinnane**

AK: We want to be talking about Albert Szent-Györgyi's work, and your work with him and what you observed about his work, but there's going to be non-work things that come in.

RP: Yes.

AK: I want to start by asking a work-related question. In your article about Szent-Györgyi for the Hungarian journal, *Biochemistry*, you mentioned that when you were a student, you had read—in fact, I think the word you used was that you had "devoured" when you found it—Szent-Györgyi's *Introduction to Sub-Molecular Biology*, a hundred and thirty-four pages, and it was an experience for you. Could you give me a sense of what it was in this book that really excited your interest?

RP: Okay, well, it's what led up to that. Because I'm not a biologist by training. I was trained in electrical engineering at the University of Southampton in England, and it was at the end of one of my second-year lectures that the professor suggested we read an original scientific paper on the invention of the transistor. It would be my first visit to the stacks in the main science library. The first journal I came across was *Nature*, which at first looked uninteresting.

But then I looked more clearly and I realized that it was like a history of science and in particular slotted down into small periods, like a month, a week, and a year, and I thought, "I wonder what happened during the week I was born?" And so I went along the shelf and I didn't reach the year I was born. In fact, I reached the year before I was born, 1941, and I still didn't know enough to know that I would have to go around the corner and find the rest. And I just randomly picked out one, and I came across an article, you know, "Zent-Giorgi" [trying to say it properly]. I didn't know it was Szent-Györgyi. It was his Koranyi Memorial lecture in 1941 and he was describing basically how semiconductors work, and energy bands, in a much clearer way than the lecturer had. And this was actually before the invention of a transistor and basically he was describing how he thought the subtlety of life was controlled at the sub-molecular level, the same kind of physics by which a transistor works, and that just made a little bit of sense to me, but I didn't think anything more about it.

I just went along and looked up Physical Revue. Found that, looked up . . . a bit more boring . . . about the invention of the transistor. But in my mind I thought, "Well, if before I was born people were already talking about living systems working at that kind of level, then by now it must be old hat and well developed," but it seemed an area that I could find interesting in, say, my future career.

I was in the Rowing Club and one morning after my twenty-first birthday I had a hangover and the guy in the boat behind me had a hangover too and we found out that we

had a birthday the same day. His name is Robert Meech and is now a recognized world expert on cell membranes and their ion channels. At that time he was doing a Ph.D. I thought, "Wow, he must be a genius if he's doing a Ph.D.," and I said, "I'd love to see what you are doing," and he took me into his lab and he basically had a cardboard box with a light in it and some snails, and he said he was just monitoring the rate at which they moved as a function of temperature.

And I thought, "Well, that's disappointing. I could do that." I said, "Is that all you are doing? I can't believe that's part of a Ph.D." And he said, "Ah, what I'm also doing is measuring what is called the membrane potential across their brains," and he showed me, and I saw for the first time micro-electrodes, and he was showing me this action potential and voltage signals. And I said, "Well, what's going on here? What exactly is a membrane made of?" And he said, "Well, you know, things like carbon." And I said, "Ah, it's the same kind of chemical group as silicon and germanium." And I said, "And what are you doing on either side?" He said, "I'm altering the potassium level and the sodium level," and whatever, and I just related these to simple analogies used in semiconductor physics, the doping of semi-conductors.

And I just happened to say something like, "Ah, I bet if you increase the sodium, the potential goes down. If you add the chlorine it goes up," and he says, "Well, how did you know that? That's exactly what we're finding." I said, "That's the kind of thing we're taught in engineering, and I was reading a paper the other day and thought that this is the

kind of thing you must already do in biology." And he arranged for an interview with his supervisor, who said, "Well, what are the theories you were using to predict our results?"

So I explained all that and he just sat back and said, "Well no, ion channels are like tunnels. Some are big, some are small, some let big ions in, some let little ions in and there's kind of like an arm at the end that stops them." And I said something which really upset him, which was "I can't believe that your thoughts, imagination, the things that you do, your body, is controlled by mechanical things. It has to be more subtle, like electronic." And he leaned back and he said "Well, as an engineer you won't know about these things. Hodgkin and Huxley, for example, had done all this and they have just got the Nobel Prize." And I left just feeling totally shocked that biology had somehow ignored what had seemed a good idea.

And so I tracked down a copy of that book by Szent-Györgyi, and it just amazed me. And then by chance, I was in Oklahoma in 1975, in August, and there was a journal lying there, *Industry and Chemistry News*, and it was reporting on a meeting that the Pope had arranged for scientists and Nobel laureates. Szent-Györgyi had given a paper and it was just totally condemned by some people and I couldn't believe that, so I wrote to him. By chance, the person who was the chairman of the board of trustees here at the MBL, Dennis Robinson, had been a professor of electrical engineering in the UK, developing radar, and was then over in the United States. And I had written a paper mentioning

Szent-Györgyi's name, and he had been up to see Prof and told him, "Look, someone actually believes in what you are doing."

And so, by the time I'd written to Szent-Györgyi, saying, "You don't know me but," in fact, by sheer chance, someone had given him my paper, and so he invited me instead of flying directly home from Oklahoma to go via Woods Hole. And, I think, three weeks later, I was back, because, he was just doing exciting things.

AK: Yes. I want to ask about your first meeting with him.

RP: Right.

AK: But before we do that, I'm struck by this remarkable coincidence of you going into the stacks to look for, was it the *Physical Review*?

RP: Yes, and it was because I was drifting and percolating down the stacks and discovering for the first time, something I love doing now, but I was discovering the joys of a library for the first time.

AK: But you were looking for an article on transistors.

RP: Yes.

AK: And you ended up wandering into *Nature* and found . . .

RP: His Koranyi Memorial lecture, which basically said there is no way you can understand the subtlety of living systems, at the gross level, that you have to go down to the sub-molecular. That didn't seem a particularly amazing thing to say. But I realized afterward that it wasn't mainstream biology.

AK: Right.

RP: And here was a biochemist who had obviously read books by Mott and Gurney. He understood quantum mechanics, at a superficial level, but at a level that really was a basic understanding. You can write down the equations but not really know what that really means, but he knew.

AK: So you came to Woods Hole?

RP: Yes.

AK: And that was the first time you met Szent-Györgyi?

RP: Yes. Woods Hole, I had no idea. The name "Hole" implied it might not be a particularly brilliant place because that's a derogatory term in the UK. [laughs]

AK: It is here, too. So tell me about your first meeting. What was your first impression of him? I gather he picked you up at Falmouth airport.

RP: It was a strange flight, in fog. We finally came down and I don't know if it's changed now, but you stepped out of the small plane and you headed for a fence. And there were a few people there and Szent-Györgyi instantly knew who I was and he was trying to avoid somebody that he had spotted who he knew would start boring him or something. He'd say, "We want to go this way" or "keep this way"—he took me to his enormous car and was talking all the time about his work; took me to a restaurant and was scribbling on napkins about charge transfer reactions.

We came here and he just pointed to this library which I now love. It is the most amazing library in the world that I've ever visited. There might be somewhere better, but I haven't found it yet. "So if you worked here," he said, "you'd have a fantastic library," and then he took me straight into the lab. And the only other person in the lab was Jane, Jane McLaughlin. And basically he just started to mix . . . he didn't believe in using big equipment, he loved test tubes and looking at colors, basically just put caseine—a protein, into a suspension. I think he probably mixed it with methylglyoxal. It went a burgundy color. He then put it into a centrifuge, spun it, and put on some huge gloves because he was just impatient because he couldn't wait for the centrifuge to slow down, so he put his hands on it—slowed it down with his hands, with his big gloves on.

AK: So he wouldn't get burned.

RP: Yes. So I was already aware that he was someone who was pretty impatient. When he was excited, he couldn't wait to find the result and he showed me these proteins which were black. Now if you were brought up in physics, you understand why things look the colors they do. Black implies that light is going in and exciting electrons up into a whole range of delocalized levels. That's why it's black. If it was transparent, for example, like the window over there, the energy gap that electrons have to go through and then drop down straight away is beyond what we see with the visible colors. Black casein could imply that you had unpaired electrons that are able to move freely through the structure, which was along some of the concepts being explored by Szent-Györgyi.

In the UK I'd had a student working on his PhD, Peter Gascoyne, on charge transfer complexes, which was an important topic in physical chemistry then. It's not so important now, but it's coming back because of the advances made in molecular electronics. Now it's taken for granted that we can make organic compounds that are highly conducting and they actually form the basis of technology, making Japanese industry, particularly, quite wealthy. They are conducting black materials. We were only making very pale purple proteins at the best, and here he was making black ones in a very gentle way. He wasn't burning them; in other words, it was purely by chemical reactions.

AK: So here you are in his lab.

RP: Right.

AK: You've landed into this small tornado of creative thinking and he's right away "I'm not much for small talk." He's right away into the . . .

RP: Well, when it came to experiments, a typical example would be that after I'd been coming here a few times and other people were able to come here to work with him because of what the National Foundation for Cancer Research was able to do, there was a young student from the UK who came who was trained in biochemistry and Szent-Györgyi said "I'd like you to do this experiment. Basically, what I'd like you to do is just measure the absorptions, the spectra of this compound as a function of pH." This guy spent about two hours carefully weighing things, getting the molarities right, and then he carefully titrated all his pH stuff, you see, and he had it all there, ready to do and then he went off to Swope for lunch.

And then Szent-Györgyi just said "Oh, God, I can't stand it, I can't stand it. Look what he's doing. Look what he's doing." And basically what Szent-Györgyi did was just get hold of a spatula and said "That's about half a gram. That's about one gram. That's about one and a half. That's about two. That's about three," and then pH, he just had litmus paper. Well that's about four. Put a bit of HCl, you know, that's about three." Within five minutes, he'd basically got the experiment showing all the colors and he realized that the idea wasn't going to work.

And so when the guy came back, he said to Prof, "Right, Prof. I'm ready now. By five o'clock I think I'll be getting somewhere," and Szent-Györgyi said, "Waste of time. Don't do it. Look," and he demonstrated, really, the difference really between his style and the kind of precise science that had grown up, I think, after him, and therefore brought with it the kind of mentality that wasn't able to readily see that here was someone who had insights. You know, "I can see that desk over there. I don't need to go from this table to there."

AK: Right.

RP: To the carpet, to the desk. He could see there was a desk, and he'd go straight to it, whereas present science wants you to go through everything.

AK: And I guess he—he knew, for example, with this desk analogy, he knew when it was and when it was not important to get out a measuring instrument to see how many meters it would be between.

RP: Well, not always. He wouldn't really have a good basis for believing it, but if he ended up finding out that that wasn't a desk, but was a bed, then he would laugh. He would say, "That's amusing." In the period between when I first came here and I came three or five weeks later, I went to—must have given a lecture or something at Durham University and I stayed in one of the halls, and it's like a mini-Oxford and Cambridge the way they were

running it. And the warden of the hall ran a high table at dinner. He sat me next to himself on the right-hand side and he was a biochemist and I happened to say that I was very excited because I had just met Szent-Györgyi and was going to go back to his lab. And he said to me, "Don't do it. You'll ruin your reputation." [laughs] He just warned me not to do that, and in fact, that kind of statement made me—I equated that with the kind of negativity I'd seen in the journal. I grew to understand what it was about, but it made me more determined than ever to sort of somehow . . .

AK: You grew to understand what that was about, but at the time that you heard it there at the University of Durham, you didn't know what it was about.

RP: No, it's either the inexperience of youth, the arrogance of youth, the enthusiasm of youth. There's a difference, and at that stage I was excited in science for science sake. It was a bit like Kennedy's speech. I wasn't asking what science could do for me, I was trying to do something for science, and I still believed that if you found something very interesting, other people in the field would say, "Wow, that's really cool." It was a slow path to realize that a lot of people make their careers out of science. It's that way 'round. You know, opposite to what Kennedy said, "Ask not what the country can do for you; ask what you can do for the country."

AK: Let me press you on this point just a little bit, on Szent-Györgyi's style as a scientist? He appreciated, or did he not, the desirability of precision? My understanding has been that

he appreciated precision and measurement, carefulness, if it were necessary, but you're suggesting something a little different about his style. That he operated on an intuitive sense of the materials at hand, in a way that was a bit outside the mainstream ways of conducting scientific research?

RP: Okay, various things have been triggered off in my head. The first thing I have to say is of course I first met him when he was eighty, eighty-two. Nobel Prize, Lasker Award, you know, so I can't comment on what he was like in the 1930s, apart from the observation that in his early career there were two lines of thought as to which was most important in biology, oxidation or reduction. You had the Warburg side, I think they're the hydrogen people, and you had the oxygen people, and they were two camps. You either were in one or the other, or you were doing something different. You weren't in that subject.

AK: This was the Warburg-Wieland dispute.

RP: Yes, that's right, and Szent-Györgyi stood back and said, "Well, they're both right. It's both." So he wasn't really influenced by what other people were thinking. He would sit back and he'd think about it for himself, and once he got a basic idea, off he would go. He told me that he started to think about sub-molecular biology when he was camping, I think in Cornwall, England, in a tent and they had a cat with them. The cat just suddenly shot up in the air because it had seen a snake, and he said there was no way that the whole

reaction, that cat, everything in that cat, would turn on at once. There was no way that that could be related to anything mechanical. It had to be sub-molecular.

And other people, significant people, I think recognized this. One of my most valuable possessions, and I guess it will have to come to somewhere like the MBL one day, and I think I might have sent you . . .

AK: Yes, you sent me a PowerPoint representation of the Monod dice.

RP: Right. Szent-Györgyi's attitude towards me was very nice. He would invite me each summer. I'd stay in his cottage at Penzance Point and I had stayed there the first evening I ever came to Woods Hole. We'll get back to that. That is a famous building, actually. The people who own it now I don't think quite understand to what extent it is. He would say, you know, "Treat my lab as a playground," and he'd ask me, "What are you interested in now? What would you like to do? Use these toys as a playground, have fun," because if something wasn't fun, there's probably something wrong with it, which is true.

I tell my students that now. If you're starting to do a project and it becomes tedious, boring, you have to drag yourself in the lab to do yet one more experiment, there's something wrong. That isn't worth doing. There's something wrong with the science. There's something wrong with the experiment. It has to be exciting and fun because that means you're doing something new, something exciting, something unexpected.

So I said to him, "Well, I've become interested in allosteric proteins, Prof," and he always used to—it was like when I first came, he'd ask me a question. I'd reply and then he'd come back and say, "Well, what do you mean?" He actually knew what I meant, but he was testing your level of understanding, actually. I said, "Well, a good example is hemoglobin. It's got these four myoglobin units and yet when an oxygen lands on one of these four units, the other three know it's happened, and grab hold. There's a communication going on between the whole thing," I said, and "I think that's an example of your sub-molecular biology. It might be something. We've got to do something that other people can relate to and say 'ah.' We've got to use your theories to predict something and do an experiment." That's what our job was really, to try as much as possible to keep this hydrogen balloon called Szent-Györgyi firmly anchored to the ground.

So he opened his desk and he brought out these dice and he said, "Do you mean something like this?" and he clicked them and I said, "Wow, that's exactly what I mean. You mean, you've done that. All four of them are moving as one," and he said, "Ah, this is one of my most valuable possessions" and then he told me the history of who had given it to him, you know, the Nobel laureate Jacques Monod, who worked out allosteric transitions. He told me some other little things about that meeting with Monod in Paris, which showed the great sense of humor between two great scientists.

AK: Had you been familiar with this dice?

RP: No, didn't know it existed and a few summers later he wanted to give them to me. He said, "If they're just found, nobody will know that they're significant." You know, Monod's dice. He left a message for Jane to give them to me after he passed away. So Monod gave him the dice because he said, "Look, this is at the mechanical level. The true understanding of what's going on here is at your level. Your sub-molecular biology level," so people like Jacques Monod could see, could sense what Szent-Györgyi was trying to do. Monod actually probably gave him something he valued himself quite a lot because I checked with Max Perutz. I showed him a photograph of the dice and he knew of them because Monod was using Perutz's work to build up his theory.

AK: You know, your description of Szent-Györgyi's methods reminds me of a story in Ralph Moss's biography, about somebody asking Szent-Györgyi how it was that he knew that a 50 percent glycerin solution would be something that might preserve muscle, so that at any point after it had been excised from the animal, with the addition of ATP, it would contract—how did he know that? He said, "Well, I just knew it." "But why a fifty percent solution?" "Well, it seemed like the right amount," but he was right.

RP: Right, but other times he was wrong on things, but it wouldn't upset him, which of course nowadays is not a too brilliant thing to have young scientists get used to because they rapidly learn that if you make one mistake or two mistakes, you're never going to get a grant. I know there are people—I won't say who they are—a bit older than me, who probably consider me to be a total idiot because of some of the ideas that we were trying

and I know of them in a few instances and I won't go into it because it would not be right of me. These people are still alive and they did what they thought was right. But I know that they had been referees on certain things and that that's been a stumbling block.

AK: Well, many people don't understand that there are politics in science, and that it isn't all objectivity and clarity. There are schools of thought and persuasions.

RP: Well, people make science their career and nowadays to make science your career, you need to go where decision making takes place, i.e., on committees that award grants and I have to say now in the UK, in particular in engineering and physics, EPSRC, there's someone I happen to know. He's been a previous colleague and he's now the head of the EPSRC. He's the CEO, the director of that grant awarding body, government grant awarding, and he's trying to get rid of the kind of committee schemes whereby committees could be self-serving, award grants to each other and exclude others. He's trying to do that, so it is recognized, but it's how you handle that because if you really want to spend a country's taxpayer money wisely, you need experts to judge it.

I made it a rule when I was in a committee never to submit a grant to that committee.

That was not a very a good thing to do in one instance, but I just felt that you should not be part of the decision making process. I mean, they did it by the person going out of the room, but they'd get the grant and it was someone else's turn to leave the room and that was how it worked.

AK: Tell me again—you came to Woods Hole. You've met Szent-Györgyi. How did it come to pass then that you kept returning? What agreement did you reach? How were you able to do that?

RP: Okay. Well, Szent-Györgyi actually was very good at attracting the people he wanted. He had an enormous charisma. I think it's why he ended up marrying someone, Marcia, fifty years younger than himself. On my first trip to Woods Hole, he let me stay in his cottage. I didn't sleep much because he just happened to mention the kind of people that had stayed in that cottage. Einstein's son had stayed there. Nobel laureates. Gamow. Watson had written part of one of his books in the cottage. In one of his books he thanks Szent-Györgyi for a room with a view over the sea. That's Buzzard's Bay. He'd given me various papers and things to read and I couldn't sleep, right by the beach, in this cottage, and I was already up at about six o'clock in the morning and reading this stuff when Szent-Györgyi came down for his morning swim.

That was the kind of energy Szent-Györgyi appreciated. It's not my normal style. I have to say I'm not very good at getting up at six o'clock to start work, and he was up for his morning swim and he happened to go past and look through the window. He invited me outside, and then at breakfast he just said, "I'd like you to come and work with me," and I figured that was just amazing, but I'd already got the feeling that his financial situation here wasn't good. Also, the kind of work that needed to be done to investigate what he was trying to prove, he didn't have the equipment. He didn't have the material sciences, if

you like, the investigative equipment, the physics. Nor did the MBL. His electron spin resonance machine had mostly been thrown away. Only parts like the magnet that was too heavy to move remained. In fact, it's immediately underneath where we are now, Room 101 in the basement.

AK: This is 1975?

RP: Yes, and he had some money left in a British bank account for air fares and he said he wanted me to come back and I said, "Well, I'll come back with my best Ph.D. student, Peter Gascoyne, and we'll get that electron spin resonance machine going." I didn't realize just what a job it would be. I'd seen it and I'd worked with an electron spin resonance machine in the chemistry department at Nottingham, so I recognized it. I understood how the thing worked so I reckoned I could put it together again. I wouldn't have been so good at analyzing the spectra, but I would know how to get them.

And when we came back here, Peter and I, we were immediately excited by this place and the enthusiasm of the people in MBL. I was very sorry to see that Ted MacNichol has just died—there was a memorial service for him yesterday. He was in the lab opposite Szent-Györgyi's. He had all kinds of electrical bits and pieces, transformers, and he just said, "Look, anything you want here, you can use." People who run the electrical workshops here were also helpful. We rummaged in their scrap metal and found bits of the wave guide that obviously had come from this electron spin resonance machine.

There was an energy, something about the people in the MBL then and I think it's still here now, how you value certain things that don't have much material value, but are valued because of their connections with science and ideas. So I was caught up in the enthusiasm of youth, but realizing that the best thing to do was to somehow have us working in the University of Wales, where we had the equipment, and somehow keeping in touch with Prof and using that facility to work with Prof. The idea of this Laboratory Without Walls was actually a concept that we had. Let's have a laboratory, but no walls, and that was the kind of concept he loved.

AK: This conception of the idea of a laboratory without walls and being connected with Woods Hole through your laboratory at the University of Wales, Bangor, was it prior to the National Foundation for Cancer Research funding that idea? Or was it about the same time?

RP: Yes and no. No in the sense that Frank Salisbury and Tamara Salisbury had already come to Woods Hole and met Prof and I think they'd already set up a foundation based in Massachusetts. Then there was a meeting organized by Benny Kaminer, who just passed away a year or so ago. He organized a meeting in Boston in Szent-Györgyi's honor for his 80th birthday. Basically, we were working like mad here to get the electron spin resonance machine going so Szent-Györgyi could go and show his fantastic colors and we wanted to give him some evidence that there indeed were free electrons. And we did that for him, but of course that's a big jump from saying anything about having a cure for

cancer, which is where he was leading. We were just going through the boring step-by-step procedures, trying to give him solid scientific evidence, but not standing up and saying, "Hey, we've got a cure for cancer." That's what, in his mind, he was linking. We were way behind that.

And then at that meeting, Hans Krebs was actually sitting next to me and on my other side were two, you know, elderly ladies. Probably seventies or their eighties, and I'd just been talking to them and they knew who Hans Krebs was and I'd never met him before. At the end of Szent-Györgyi's lecture he said to me, "Do you believe any of this stuff?" I wasn't cowed by this. I said, "Well, yes, some of these are basic and fundamental ideas. I'm uncertain of the big jumps he's making, but the idea he's talking about, delocalized electrons," I said, "that—I really do believe that. I really believe that is an area worthy of exploration for itself, not necessarily that it will lead to the cure for cancer."

AK: This is Krebs asking you do you believe in the stuff and at this point you still don't know he's Hans Krebs?

RP: I knew he was Hans Krebs then, and these ladies apparently were impressed by the fact that I wasn't cowed by Hans Krebs and that I came straight back to him and said, "Yes," and I think this is what they were wanting to hear because they asked me, "What are you doing here?" I said, "Well, I'm actually working in Szent-Györgyi's laboratory." "Wow. How many people has he got working with him?" and I said, "Well, basically, it's one

person all the time and I'm here with someone else," and I pointed Peter out, "for a couple more weeks." "Why for a couple more weeks?" they asked. I said, "Well, Szent-Györgyi doesn't really have the money to pay salaries or to support people and so we're interacting with him and finding money for airfares."

What I didn't realize was that they then sent a check, I think it was for fifteen thousand dollars, to Szent-Györgyi because in one of his letters he says, "The little old ladies that you met," or something like that, "sent me some money, so we'll have plenty of money for airfares." What Frank Salisbury then did was to use that money for postage and to send out the first mailing slot, was how I understood it from Frank Salisbury.

AK: These were fund-raising campaigns through the mail that Salisbury was directing?

RP: Yes. So those first lot of funds came in. The money from those ladies was multiplied and then Frank Salisbury started to talk to me about what we were doing and I told him about the idea of a laboratory without walls and he and Tamara came across to Wales and we were set up as the very first workshop.

AK: And these are the days, I think, before there was email, before the internet.

RP: Oh, yes.

AK: Airfares were the way . . .

RP: And we exchanged information by telegrams. When we did the first electron spin resonance, we sent him back a telegram saying, "We've got a signal," and he sent back a telegram saying "Fantastic."

AK: Have these telegrams been preserved?

RP: Well, of course, no, I don't have his telegrams, but what I've got are his letters written back saying, you know, "Exciting news." We ended up—well, we must have started getting money pretty quickly because there was a time when there was hardly a period when there wasn't someone from Bangor, one of my Ph.D. students, actually working with Prof, and they were just very excited by the stimulation that he provided.

AK: I remember reading in the June 1987 article that you wrote for the Hungarian journal, that at the time you became interested in Szent-Györgyi's work, when you read his *Introduction to Sub-Molecular Biology*, of course, you had not known anything about his Nobel Prize.

RP: Oh, no, I had no idea at all.

AK: So you entered into—you joined Szent-Györgyi's thinking at this sub-molecular level and then discovered that he had . . .

RP: Yes. Well, I mean his Koranyi Memorial Lecture spoke for itself.

AK: You were drawn to him purely on the basis of his ideas.

RP: Yes.

AK: And not—I mean, you didn't know anything about his status as a scientist.

RP: No. In fact, that might have prevented me writing to him. Well, I think I knew when I wrote to him because it implied that it was a meeting for Nobel Laureates and I think it was then that I realized that he'd got a Nobel Prize and it was then I looked him up and saw that it was for vitamin C. So I was a bit nervous when I actually came to meet him for the first time, but up until then I had started a research program and had written an article mentioning Szent-Györgyi's ideas.

AK: You came back here how frequently?

RP: Well, quite frequently. I know that's not very quantitative. As often as I could.

AK: So certainly more than just a summer.

RP: Yes, about ten summers and other periods besides that. I was very lucky because the person who was the head of the University of Wales was Sir Charles Evans. He was the first person to climb the South Peak of Everest. He was the medic and the second leader in that successful 1953 Everest expedition. You see, Charles Evans was an outstanding person and he had grown up to sort of worship Szent-Györgyi. You know, in his training Szent-Györgyi figured largely in the development of medicine.

What Szent-Györgyi had done after I had visited, he had contacted a guy, I think Slater who was high up, it might have been the chancellor of Boston University, and they worked out a position for me at Boston University so I could work here. I had just had a phone call from Boston University and I'd been called out from a Board of Studies meeting in Bangor to take it, and I was quite rattled by that. Then the phone rang and it was Charles Evans calling me and I still don't know why he called me at that point.

Whether or not someone from Boston University had, you know, phoned through the administration to find out where I was or something, I don't know, and he sensed I was in a bit of an emotional state, and he just said, "I'm in my house right now. Please come and talk to me. I want to find out what's causing this emotional stress you seem to be having at the moment."

I took along a couple of these vials that Szent-Györgyi had sent to do the electron spin resonance measurements on and, you know, I was a pretty new lecturer at this university.

Why is the vice chancellor inviting me to his house? He was in a wheelchair, probably related to the exertions of going up Everest. He and Tom Bourdillon should have been the two who conquered Everest, but he ran out of oxygen. I showed Charles Evans these things and I said, "Why do you think these are these colors?" and he said, "I don't know. It's got something to do with the way the light interacts with them," and I explained to him why I was excited and from then on he basically did everything behind the scenes to let me free up my time for the U.S., to spend whenever I could here in Woods Hole.

So there was that support from the university because the man at the top adored Szent-Györgyi, worshiped him, in a sense, as a medical giant, and he told me he felt that by being able to enable me to go that somehow he was having part in that. So these are the two big things. There were the scientists, some of them telling me I'd ruined my career and there were other people I really admired. Mountaineering was something I was interested in when I was young. Charles Evans was really the reason why I wanted to go to that university because I just thought, "Wow, there's somebody, you know. What a guy to have as your vice chancellor."

AK: Another remarkable coincidence, that Evans would be vice chancellor at the university that needed to support your coming here.

RP: Yes.

AK: Was there a Boston University appointment of some kind? Is that . . .

RP: Well, there must have been plans like that involving Benny [Benjamin] Kaminer. I don't know, but it seemed to be Szent-Györgyi talking directly to Slater, and also Dennis Robinson was another factor.

AK: I see.

RP: I think there were a lot of people here wanting to support Szent-Györgyi. He obviously was relying, I think, on charity from very rich people on Penzance Point and I think the people who had supported him suddenly saw there were younger scientists who thought that what he was doing was worthwhile and they were physicists rather than biologists. I think they were kind of excited as well, and they wanted to make things happen.

AK: Your opening paragraph in this article in *Biochemistry* is quite interesting. I'd like to take it apart sentence by sentence because . . .

RP: I wrote that so long ago.

AK: I know. I'm sorry to spring something on you, but there's three sentences. I want to read each one because I think each one is a door.

RP: Right. Okay.

AK: Quote, "If he could be asked, Albert Szent-Györgyi would tell us that although his work on metabolism, ascorbic acid and muscle provided him with much satisfaction and enjoyment, the most important and far-reaching of his endeavors were those engaged in trying to understand the living state at the sub-molecular or bioelectronic level." What gave him the most satisfaction was this.

RP: Well, that's what he used to tell me.

AK: This is from his own—this is not your inference?

RP: No. When he was in Cornwall, it must have been the period he spent in Cambridge with Hopkins, and he had behind his desk and also above his bed in his bedroom in his house, a picture of Sir Gowland Hopkins, who got the Nobel Prize for discovering the first vitamins. He was the one who recognized Szent-Györgyi's work and actually got him to go to Cambridge at a point in Szent-Györgyi's life when he was about to give up science, so he said. You know, he sometimes—I learned that Prof quite liked drama. He liked putting drama into things and so he sometimes would use words to make something more

colorful than it might have been in reality because I think he was born a scientist and there was no way he was going to give up science. But anyway, he reckoned Sir Gowland Hopkins rescued him at a time when he was in a bit of a lull in his career as a scientist.

AK: According to Ralph Moss he was darn near suicidal, maybe, and starving.

RP: Well, where did he get—Ralph Moss get . . .

AK: From Szent-Györgyi.

RP: Yeah, you see, this is where you have to be perhaps a bit careful, I would say. [chuckles]
I'm just suggesting that, okay.

AK: Sure.

RP: So by the time—the Koranyi Memorial Lecture was 1941. So he'd obviously been reading. You can see that he must have been reading stuff written in 1938-39 on conduction and energy bands. Why was he doing that as a biochemist? And it would have been probably the first year he was in Cambridge, went on holiday. You know, cheap holiday, got a small car, went in a tent, camping in Cornwall. He saw this cat jump up in the air. He said *that* was when he knew.

So I think from that time on, that was really what he was after. He saw that as something really big. Vitamins, ascorbic acid was not an accident, exactly, but not as big a thing. Muscle excited him, you know, and he jumped immediately to a pigeon. Why did he use a pigeon muscle? Because he looked around. Well, what muscle would be really working really hard? Well, the muscle of a pigeon would be a good one to go for.

And Benny Kaminer said that when he came here, first of all, to work on muscle, from South Africa, he came to work on myosin, etcetera, and he was very disappointed to find that instead Szent-Györgyi was focusing on this electronic stuff. He'd got an electron spin resonance machine, and people were trying to measure free radicals at a time when free radicals weren't actually a big thing in biochemistry. I've got with me a copy of the referees' report that rejected the *Nature* paper, and the referee is saying that there have now been many instances where free radicals have been found in biological systems, but you don't jump immediately to the cure for cancer from that.

But Szent-Györgyi was looking at free radicals before other people were really focusing on that. So he really believed, you see, because there were people that the Foundation brought in who started to synthesize these compounds. We had a Mouse House just outside. It's no longer there but he had mice, inducing tumors in the stomachs, injecting these charge transfer complexes . . . it wasn't fraudulent. These were real animal studies and real longevity increases for the mice being fed these compounds . . . sometimes the

tumors disappeared totally. I still believe there was something going on there that really should have been carried on further.

Some people down in Texas actually wrote some years ago saying they'd reproduced the work but they reckoned that what these compounds were doing was triggering an immune response that wasn't known at the time we were doing that work. So what was inducing them? He wanted to know *how*.

AK: So his satisfaction, his excitement about life processes at the sub-molecular level was that here is the answer to the big question. Here's where we ought to be looking for the answer to the question, "How does it all work?"

RP: Right, because he reckoned that biology was a lot of—you know, it was look and tell largely. You look down a microscope. If you can't see it, it's not there. I mean, he's not right in saying that.

AK: Right. I think that was his reply to Krebs' criticism.

RP: And he was a bit naughty in that, actually, but he actually liked to create an atmosphere of—of, not negative interactions, but he seemed to be stimulated by having someone or something to fight. You know, like fighting the Nazis and Hitler.

AK: That's right.

RP: Or the Russians—you know.

AK: Right, I hadn't thought of that.

RP: He sometimes seemed to deliberately try and provoke something to raise the temperature to give him energy. Does that make sense?

AK: Yes.

RP: He survived in conflict, actually. [laughs]

AK: It certainly provided a great context for him to articulate his own viewpoints. Yes, that's an interesting idea.

RP: Get him up and to come in to prove them wrong.

AK: Yes, interesting insight.

RP: Well, I don't know, but that's just a feeling I had.

AK: Let's go onto your second sentence. This is three sentences in a row, but I think they're loaded. "Yet these last endeavors brought to him many frustrations and are either little known or mostly misrepresented within the scientific community." Tell me about these misrepresentations?

RP: Well, a misrepresentation that came out is in the literature now in that there was a book review written for *Nature* and it included some sentences that I objected to because the person that wrote that review—the name's right on the tip of my tongue. Interestingly enough, the copy of *Nature* with that in it was actually removed from the library here, which is interesting. I wrote a letter to John Maddox, which ended up being published, where I pointed out that what the person who wrote this thing for *Nature* said was that Szent-Györgyi . . . not misguided, misdirected, or corrupted, but he implied that he got people—he didn't mention any by name, but he . . .

AK: This isn't Walter Gratzer?

RP: Yes, that's right. He was a fine chap. I mean, I've got nothing against him, but somehow he had got the idea . . .

AK: I think 'perverted the talents' was the phrase, right?

RP: That's right. Right.

AK: And you took issue with that.

RP: Yes, and with something else . . .

AK: Szent-Györgyi's personal life in his later years . . .

RP: Yes. But why did this person in Durham say, "If you go and work with him, you'll ruin your career"? I got the feeling that he frustrated people by jumping to conclusions without proof. Even if it ended up being right. I mean I can see that. To be quite honest, I never really went there too much, because I wanted to stay positive. Because there were people here actually who wanted us to succeed and people who were significant. This is the thing, that people quite often who were the critics—I don't want to sound pompous here, but they were significant in terms of their stature, if you like, where they got to, but if you met them as people, they were not as impressive. Now, you won't have met Dennis Robinson. There was a guy who was involved in the development of radar then went on to develop linear accelerators. He set up a big company, did a lot for MBL. He was a significant person. He was a Szent-Györgyi supporter.

Benny Kaminer, even though he was bitterly disappointed when he came here, did everything he could to support Szent-Györgyi and would have loved to have seen us do well, you know, so you had those kind of people on one side and others that kind of gave rise to this "perverting talent" notion. And where did that come from?

AK: Right. That was exactly my question.

RP: I just don't know.

AK: Well, you're suggesting that perhaps one contribution might have been Szent-Györgyi's own style. That he had a way of coming to conclusions or spinning off his "big hook" ideas that disturbed some people, or bothered them.

RP: It could be that when somebody has achieved what somebody like Szent-Györgyi has achieved—and Einstein is another example because I've got a quote on my desk. You know, Einstein has made the statement that he despises those scientists who go around with a drill and drill big holes, you know, in the thinnest and softest part of the wood and avoid trying to drill holes where it's difficult. A bit of a naughty thing to say, really, because everybody should respect everybody else's style and what they want to do and if someone recognized that they're best fitted drilling holes where it's easy, then that's still an important thing to do.

AK: I can see where you didn't want to get caught up in that.

RP: Yeah. But you're right, I think Szent-Györgyi brought a lot of problems on himself by making the very statements that would turn some people on, and also turn some people off.

AK: I understand why you wouldn't want to go there and yet it's important to say, to acknowledge, as you did, that there were these tensions in the scientific community, created in part by Szent-Györgyi's style.

RP: Well, not—you see, my very first international conference was in Israel. It was on Low Mobility Solids and it had a picture of a tortoise as a symbol and low mobility solids meant those materials where the charge carriers hardly moved at all. I had experience, for example, in doing my Ph.D. at Southampton and I had already become interested in bio things and I could see that the way to go there was through molecular electronics and so I built some equipment to measure how fast—in fact, they go very slowly—the electrical charge carriers move in things like naphthalene crystals and anthracene crystals. Naphthalene is basically what mothballs are.

We had a change of professors at Southampton—the professor to begin with was very supportive. Somehow he related to me. He retired and a new guy came in from Cambridge and he went around to meet the few of us doing our Ph.Ds. and he asked me what I was doing and I said, "I'm measuring the majority charge carrier mobility in naphthalene single crystals," and he didn't know what I was talking about. He was an electrical engineer, electrical machines and motors and things, and he said, "Well, can you explain that to me in words of one syllable that would be understandable to a layman?" and I was a bit cheeky here, but what I said was actually scientifically correct. I said, "I'm measuring the effective mass of holes in mothball crystals."

The next day he called me to his office and he said he had decided that the kind of work I was doing wasn't the right kind of work and he wasn't going to support my continuation to do a Ph.D. Okay, so somehow I had upset him. In fact, it's when he saw me a few months later and said, "Well, what are you doing?" and I said, "Well, actually I'm going to Oxford to do a D.Phil. They're very interested in what I'm doing here. In fact, the work I've done here is going to be put forth as my first year and I'm really getting excited about that." And he then said, "Well, if they'll have you in Oxford, you better stay here," and he then was able behind the scenes to arrange that I couldn't go to Oxford.

So there's one example where somebody had been upset, and at this international conference I presented what now would not upset anybody, really. I actually had someone rushing down the main auditorium shaking a bit of paper at me saying, "This is a dog's dinner. This is a dog's dinner!"—referring to my measurements on proteins. Really, somehow what I was presenting had infuriated him. So there are people actually who got infuriated for reasons I never quite fully understood. I don't know.

So you could say that I upset that professor in Southampton. And I made that person rush down the hall and shake his fist and wave a paper at my very first conference abroad, really unnerving me that someone could do that while I was actually presenting a paper. Did I do that or did I just trigger off something? So there are probably two sides of this. It's an interesting—you know, you could do a Ph.D. just in that topic alone, I think.

[laughs]

AK: The sociology of science. Yes, indeed.

RP: Yes.

AK: Your last sentence in this paragraph that I want to draw you out on. "This speaks volumes of the man himself, for to him scientific research was a game to be played for its excitement, enjoyment and intellectual challenges, not for its winning of prizes or, dare I say it, the approval of others."

RP: Yes.

AK: There's much in there. Excitement. A game. Enjoying.

RP: Well, fun, yes, because it was fun. Things were fun and he did say something to me—well, he would have people coming in the lab—and I know who they are and I won't say who they are—who were trying to encourage him to nominate them for a Nobel Prize, for example, and one even came twice. You were aware that this might be what was going on, and Szent-Györgyi once made a comment to me. He said, "Once somebody like that thinks about winning a prize," he said, "he'll never get it." He said, "You don't do science for the idea of winning a prize." He said, "That's not the reason you do it. They're in science for the wrong reason," and coupled with the fact that if something went wrong, he would laugh. He said, "That's amusing." You know, it would be amusing.

After all, he was working in the lab until he was ninety-two, at the same desk, the last summer he was alive, with his test tubes and his pipettes, having fun. He would grab you across and he'd say, "Look at this color! Look at this! Look at how fast it goes!" You see, he would make a reaction between two colorless chemicals and they'd shoot to a burgundy red. That was one of his most favorite colors, but he'd then see that for a split second, a mini-second or so, he'd see a yellow color and then he'd say, "Can you see that yellow color? I think I know what that is," and then he would probably in less than a hour be there with a centrifuge doing this and that, altering the solvents. You see, not only pH but dielectric constant was something he used in separating out things.

Other chemists have done that, but it makes a lot of sense because solubility, how ions repel each other has a lot to do with dielectric properties of the surrounding medium and he had an intuitive sense of that. He would within a couple of hours, isolate a small—the end of a spatula full of that yellow material. He would have seen it and he would have isolated it. That was a challenge. That was fun and that's how he told me he discovered vitamin C. He was having paprika one night and he didn't really want it, and then he realized he'd not looked at that as a source of this substance.

AK: But he saw the yellow?

RP: Yes, he saw the yellow.

AK: And he thought, "What's that?"

RP: What's that? It's an intermediate, because for him—because you could see this actually. It was the oxidation-reduction process. I mean, what's converting your food into energy right now? Why are you breathing in oxygen? Your food actually is ending up producing a transfer of electrons down a load of proteins, cytochromes, oxidases, reductases, a little chain, and every now and then that fallen energy's captured to produce ATP. So in his mind, and a bit like the diagram I think I've drawn here [shows diagram], he thought in terms of a big energy difference because there's a huge energy difference between oxygen and hydrogen. In fact, if you mix them up wrong, you get a big bang. Biology doesn't produce a big bang. Every now and then somebody does spontaneously ignite. It's probably related to that, actually, when all these other little tiny steps are short-circuited.

So he saw it in terms of transients and so he'd see this end reaction, but he'd see there was an intermediate reaction and to him that would be the important one because it was probably reversible. It fitted into a small part of a total energy transfer scale. So I think he was still basing his ideas on the old science, if you like, of when he first started, when he first said it's neither oxidation or reduction, neither oxygen nor hydrogen, neither Wieland nor Warburg, it's both. You have to look at the whole thing because two reactions are going on at the same time, the oxygen and the hydrogen interaction, and everything in between is what's driving life.

AK: So you don't do it for the winning of prizes . . .

RP: Well, you can say that when you've got the Lasker Award and the Nobel Prize.

AK: I understand, yes.

RP: But that wasn't . . .

AK: He wasn't immune to the honors, but he didn't do it for that reason.

RP: No, he didn't.

AK: Nor, and you put this in parentheses, you say, "Dare I say, the approval of others." You put this in parentheses. What do the parentheses mean?

RP: It's probably my sensitivity to the issue of a scientific career. When I started doing science, I had a certain naiveté about what I was joining, that it was for fun, excitement. It was kind of pure. You didn't lie, you didn't cheat. You admired other people's work and you were in a kind of a club. In using the parentheses, I think I had by then become aware that if you really wanted to succeed, you had to be a politician. I want to be careful here. I think I was wary of implying that you didn't necessarily want the approval of your peers or the senior people in science. That is dangerous—you don't deliberately go out

and make enemies of all the people who control the sciences at that kind of level, if you want to succeed. I don't know whether I'm right in that. That's why I probably put it in parentheses: 'or dare I say.' There's a feeling of being a bit frightened, if you like, by actually saying that sentence. I was treading on dodgy ground. When I finish my career, I will be more blunt. [laughs]

AK: I understand. What it says to me is that this is something that the scientific community does not openly talk about with any degree of comfort. The politics of science.

RP: Because you're frightened.

AK: And he wasn't.

RP: No, he loved to go out there. I think this is one of the things he liked to do. He liked to challenge Hitler. He challenged Stalin in World War II, to be nominated to be the person to go and communicate between the British government and the Hungarian government. He was a daredevil actually, I mean, he took up water skiing when he was eighty. [laughs] He wasn't frightened. I think he had a belief that he was immortal in many ways.

AK: But you weren't frightened away from him or his research?

RP: No, because I believed and I still do that there's much valuable ground to be made in biology at the sub-molecular level.

AK: Right.

RP: And it's happening, of course, bioelectronics. There was a whole community, created by Per-Olov Löwdin who set up the International Academy of Quantum Molecular Science in 1967, which had meetings in Sanibel. He's Swedish and was on one of the Nobel committees. He admired Szent-Györgyi because . . . You see, one of Löwdin's claims to fame as a scientist was that he developed the idea of the hydrogen bond. That makes water different from any other liquid, actually. In the hydrogen bond, the proton tunneling between two atoms, hydrogen and oxygen—the bond is actually a quantum mechanical tunnel.

So he thought in those terms, and he saw Szent-Györgyi was right and at those meetings were all these quantum biologists, quantum physicists, who would not take any exception at all to Szent-Györgyi's ideas. That was, of course, the side I was on and what I didn't know was that there was the whole—Szent-Györgyi explained it to me once, the basic difference between a physicist and a biologist when they do an experiment. He said, "Now, when a physicist does an experiment one hundred times and gets an answer ninety-nine times and once he doesn't, you see ninety-nine times is the law. That is the theorem.

That is the way. When a biologist does an experiment and gets ninety-nine results and gets the one odd one," he said, "that's the one you go for."

Now, I don't know whether he was saying his kind of biology or biology in general. I actually tend to think that biologists in general are not too far away from the physicists. So . . .

AK: That was his approach.

RP: But he implied that was the difference between a biologist and a physicist, and that was what he was trying to get me to think in terms of.

AK: Right. It reminds of his saying for which he's so famous, "to see what everyone has seen, but to think what no one has thought." To look for the unusual.

RP: Yes.

AK: All that we've been talking about would have a bearing, of course, on something towards which he was famously resistant and that is applying for research grants.

RP: No, he wasn't resistant. He used to apply, but he never got them.

AK: He never got them

RP: Well, because he adopted this attitude which, if you worked with him, you could understand. You could see it is not the way to get money. You know, there's the golden rule. He who has the gold, rules, and so you've got to play by their rules. So I never ever actually was at one with him in this, but he would say, "How on earth can I apply for money to do research over a period of three years, starting in, say, six months' time, if I don't know Friday what I'm going to do until I've got the results out on Thursday?" You know, because for him research was a bit like that poem by Emily Dickinson—

"Exultation is the going.
Of an inland soul to sea—
Past the houses, past the headlands
Into deep eternity!

Excitement for him was to set off to discover the United States like Columbus. You head off into the unknown, knowing that there's something there, the big picture.

AK: Of course, he always got some funding eventually. Somebody saved him.

RP: Yes, he had a charmed life in that respect.

AK: The poem . . .

RP: Well, I never showed him that. I often would wonder whether he would relate to it because science was an excitement for him, an exploration into the unknown.

AK: Well, it's appropriate because here he is by the sea.

RP: You cut open an apple, it goes brown. You cut a banana, it goes brown. An orange doesn't. Why does the apple go brown? Why doesn't the orange? Everybody's seen that. He said, "Ah, there must be something in citrus fruits that prevents oxidation. Wonder what that is? I'll look for it." He wasn't looking for Vitamin C. He was just trying to see what made some fruits resist being exposed to oxygen. [chuckles]

AK: He wrote the following in 1943. Now, this was during the war and so it has to be taken in context, but "Scientific research is a passion. The real scientist is driven by this passion, and is ready to bear privation and if need be, starvation, rather than let anyone dictate to him which direction his work must take." Now, I mean this is partly a reaction to Nazism and as time would develop it also became a reaction to Stalinism, but nevertheless, I think it does reflect his attitude, wherever he was. It sets a very high standard of dedication for a scientist, to be willing to risk starvation and, again, with the possibility that he was over-dramatizing, he certainly did undergo some difficulties. Maybe he didn't have any choice, in terms of his temperament and everything else. He had to be a scientist. Did he ever speak to you about the idea of commitment to science? Did you talk about this topic, the level of personal commitment that it takes?

RP: Well, it was always there, really. You didn't really have to . . .

AK: It was enacted.

RP: Yes. I think you're right. He wouldn't go to the level of starvation, but he certainly wouldn't stop doing a line of work because he was told by someone else it wasn't right. I mean, he got into trouble with Nixon, of course. One big problem that he had was in his taxes. Do you know that he invented the continuous centrifuge and he was using the money from royalties to look after Hungarian scientists, and he told me that he was told he could claim against his taxes on that?

AK: Steven Rath told him it was tax deductible because even though his royalties were private income his institute at Woods Hole was a nonprofit institution, which was bad advice.

RP: But he blamed Nixon for that because he would write papers and then after it had been approved, he would get to the stage where he was doing the proofs, and he would insert a sentence like, "If more money was spent on this kind of research, rather than the immoral napalming of Vietnam, the world would be a better place," and he reckoned that that upset people.

AK: I imagine it did.

RP: And they said, "Get this guy." I mean, I don't know whether that happened, but that . . .

AK: So he created situations.

RP: Yes, definitely, and I wouldn't advise that for any young scientist starting up . . . because that's mixing politics and science, which is really interesting because he would say you shouldn't mix politics and science, when he actually did the opposite.

AK: Well, one pays a price for that.

RP: But he would groan a bit about it. If he had to pay the price, he would actually complain about it. That's quite an interesting statement, when you actually analyze it.

AK: You know, you described him in an image that's been on my mind now ever since you said it, so I'm going to ask you about it. You described him as this large hydrogen balloon.

RP: Oh, right.

AK: And that a number of people were trying to tether this thing, obviously of value, but in need of grounding.

RP: Well, that's probably an arrogant statement to make, but I saw it as the job that we had—to listen to his ideas, you know, and then to pick on a bit and say, "Well, what can we do here that fits in the logic of the big picture, but to build it up piece by piece and put it on a scientific basis that could be published in the literature," because curing cancer quite frankly is such an important topic, if someone's right, you can't risk waving your arms around and saying, "It's because we believe it's true. God exists," sort of thing, kind of big picture. I mean, people believe in God and they think they have all kinds of evidence for that. In fact, one of the most amazing experiences of my life touches on this because he didn't.

When I was staying with him one winter down in Florida, he invited Paul Dirac, and his wife, who is a daughter of Wigner, the famous physicist. Dirac got the Nobel Prize when he was in his thirties, an English guy, born in Bristol. I didn't know at the time that there was the term Dirac, which was one word per year, which is a unit they used in Cambridge. He came for dinner. So Dirac's sitting here. Szent-Györgyi is sitting here, and I'm here. Szent-Györgyi's wife, Marcia, is there, and Szent-Györgyi says to me, "Explain to Paul my bioelectronic ideas." Okay, so I said, "Well, in a nutshell basically Szent-Györgyi's saying that we tend to divide the physical world into four things, you know. We've got gases, liquids, solids and plasmas, four things.

But Szent-Györgyi would say there's the animate and the inanimate and you can tell the difference between something that's alive like my hand and this table that's dead using the

laws of physics, chemistry, using all the laws that define gasses, liquids, solids and plasmas. You could use those laws to describe the living state, and that's where he starts from and that's what we're trying to do," and Dirac just said, "Ah, there's only one thing you need to know." He said, "There is no way physics, chemistry, any of those laws can help that problem. There's only one thing you need to know, and that is that God breathed life into planet earth. End of statement."

Well, after Dirac went, and we're talking here about one of the big giants of science and a physicist, and most physicists don't really need God. They reckon they can explain everything. Most biologists here look down a microscope and can see the miracle of life, leading them perhaps to believe in God. After Dirac had gone, Szent-Györgyi said to me, "Dirac's a stupid man. He's really stupid," and I found that quite amazing, and we actually did touch on his religious attitudes in the last few months of his life, sitting there, when he knew he was dying and we knew he was. So he didn't need God in his thinking.

And yet someone like Dirac, who conceived anti-matter, and an equation that can explain everything in the universe, to him God was a reality. I found that very interesting. Dirac was very humble in that way, but really thought big. Szent-Györgyi in a way didn't need God. I found that an amazing experience.

AK: Szent-Györgyi was on an American talk show once. He was asked somewhat naively if he were a religious man. This was in Moss's biography, so this must have again come from Szent-Györgyi. He said, "There's a difference between a religious man and a pious

man. The religious man says, 'I believe in God and I know what God says and my God is better than your God.' A pious man says, 'Isn't it all wonderful? Where did it all come from? What's it all about?' He has a sense of awe about nature and life." So he may have been a materialist, and having a sense of awe doesn't make you religious, but I suppose he was saying "I'm a pious man." What do you think?

RP: Oh yes, because basically he would say to me he's trying to understand the miracle of life, so he used Biblical terms in the word "miracle," and that was to him a very big question, "What is life?"

AK: Was religion part of the discussion that would occur informally among people?

RP: No. You see, Jane [McLaughlin] was very religious. The only time that religion came up was when I would have private conversations with him. You know, we were basically talking about his dying and I was asking him these kinds of questions and I'd say, "Well, where do you think your intellect came from?" I said to him, "When people die, all of that insight, all of that knowledge, the solid is still there, but this goes . . . where do you think all of this comes from?" And he just kind of looked up and he said, "From up there, I suppose." So, yeah, who knows what he really, really . . .

AK: Who knows. Well, his position was essentially somewhat agnostic and therefore one is not obliged to know.

RP: Yes.

AK: By definition.

RP: Which of course is that little funny game he had with naming vitamin C.

AK: Yes, "Ignose" or "Godnose."

RP: Yes. [laughs]

AK: Another example of his thumbing his nose at the authorities, you know.

RP: Exactly.

AK: We actually have covered most of the territory I wanted to cover with you, but I would like to ask is there anything, an anecdote, an observation, thoughts that you have about your experiences with Szent-Györgyi, your work with Szent-Györgyi, his work, that we haven't covered that you might want others to know about?

RP: Well, here is an example of his sense of humor. About the last thing he said to me was:

"In Hungary we used to say—everything has an end—except a sausage—which has two!"

But anything else is probably very private, valued things that involve—because my family

spent time here, so there's quite a lot of things . . . and I wouldn't want to get into areas covered in Moss's book, which I got involved with a bit after, which was the fight, if you like, after he died.

AK: I understand.

RP: Because I never saw anything directly that could support any of that.

AK: Okay. The other thing that you have mentioned that I hope is followed up somewhere here at Woods Hole is the historical value of Seven Winds, his house at Penzance Point.

RP: Yes, because I started to write to people who have stayed there and I started to write a kind of a little story about The Pebble at Seven Winds.

AK: This is the cottage?

RP: Yes, because some quite significant people in science stayed there. I feel very privileged. Some of those I've written up—the trick Watson played on Gamow. Szent-Györgyi was not very happy with that. He thought that was abusing one of his guests.

AK: It was. [laughter] Well, thank you so much for this. It's been enormously helpful, in terms of getting a sense of, not only Szent-Györgyi as a scientist, but as a person and as

somebody who you obviously were very, very close with. Thanks for sharing your experiences.

[End of Interview]

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